ABSTRACT
Considerable debate exists about what analyst experience measures and whether analysts learn from their experiences. Extant research has argued that once innate ability is considered, analysts’ general and firm-specific experiences are not relevant to understanding their forecasting performance. We argue that measures of experience need to be expanded to also include task-specific experience. Our results reveal that analysts’ forecast accuracy is associated with both their innate ability and task-specific experience. In addition, we find that forecast accuracy and task-specific experience are most highly correlated for those analysts who survive the longest and, thus, presumably have the greatest innate abilities.

ACKNOWLEDGEMENTS
We thank an anonymous reviewer, Ery Black, Sarah Bonner, Larry Brown, Ted Christensen, Bill Cready, Chris Hogan, Bob Holthausen, John Jacob, Peter Johnson, Bob Libby, Lynn Rees, K. Sivaramakrishnan, and Kay Stice for their valuable comments. Previous versions of this paper were presented at workshops at Georgia State University, Oklahoma State University, Texas A&M University, and Brigham Young University. We also thank I/B/E/S for providing EPS forecast data.

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The Roles of Task-Specific Forecasting Experience and Innate Ability in Understanding Analyst Forecasting Performance

1. Introduction

Prior empirical research in the analyst domain is inconclusive on the link between experience and forecasting performance. This research has largely focused on general forecasting experience as an analyst as well as forecasting experience with a specific firm. Using the latter metric, Mikhail, Walther, and Willis (1997, 132) report that analysts’ forecast accuracy improves with firm-specific experience which they define as the number of prior quarters an analyst has issued a forecast for a particular firm. On the other hand, Jacob, Lys, and Neal (1999, 53) report, that after controlling for innate ability, no firm-specific experience effects can be discerned. Jacob, et al. conclude that only the highest-ability analysts survive at the job and, as a result, performance is not a function of analysts learning from their general or firm-specific forecasting experiences. This conclusion is somewhat perplexing as it is in contrast to substantial research in psychology and other applied fields which suggests that both innate ability and experience are relevant to understanding human performance. The purpose of this study is to explore the roles of innate ability and a particular type of experience—namely, task-specific experience—in explaining analyst forecasting performance.

We believe it is important to study the roles of learning-from-experience and innate ability for at least two reasons. First, as suggested above, there is considerable debate in the literature about what analyst experience measures and whether analysts learn from their prior experiences at forecasting (Beaver, 2002). Given the large potential rewards accruing to the high-performing analysts and the firms that employ them (Stickel, 1992; Mikhail, Walther, and Willis, 1999), it is important to understand what causes analysts to become accurate forecasters. Further, by better understanding the different skills and experiences that might be relevant to analyst performance, investors may be able to better predict when analysts will be able to perform at a high level and when they will not. Second, understanding what leads to high analyst performance could influence how their employers select and train those analysts. That is, if analyst performance is largely a function of innate ability, then firms that employ analysts would be
most concerned with identifying those potential analysts with the highest innate ability and less concerned with their subsequent experiences. In contrast, if analyst performance is significantly determined by content learned from on-the-job experiences, then these firms would be concerned with providing the appropriate experiences and training to facilitate analyst learning and, thus, high performance.

In this paper, we draw on psychology-based research and hypothesize that both learning-from-experience and innate ability are likely to be relevant to explaining analysts’ performance. In doing so, we argue that a previously unstudied aspect of analysts’ experience – task-specific experience – may be the key to understanding why prior research found no evidence of learning from firm-specific experience once innate ability was considered. We define task-specific experience as the analyst’s experience in forecasting around a particular kind of situation or event (what we refer to as the task), such as forecasting earnings when restructurings occur or forecasting earnings around an acquisition. Prior research focuses on general and firm-specific experience which we argue are unlikely to create the knowledge and skills that will transfer to facilitate high forecasting performance in all situations encountered by the analyst (Bonner, 2006; Singley and Anderson, 1989; Gick and Holyoak, 1983; Marchant, 1989). Although an analyst may have many years of general analyst experience and also may have followed a particular firm for many years, s/he may have no experience in a particular task situation. Said another way, analysts with the same level of general and firm-specific experience are likely to have vastly different task-specific experiences through which they acquire different knowledge and skills (Bonner and Lewis, 1990, 2).

We test our ideas by studying analyst forecasting performance within the context of firms that experience restructuring charges from downsizing (i.e., exiting an activity). We study whether prior experience with firms that undertake downsizing restructurings will improve analyst forecasting performance around subsequent situations involving restructurings. Employing a cross-sectional analysis, we regress analysts’ relative forecast errors on general, firm-specific, task-specific experience, innate ability, and control variables that have been previously identified in the literature.

Our paper reveals two new insights. First, our results suggest that both innate ability and task-specific restructuring experience are important in explaining analysts’ forecast errors around subsequent
restructurings. Even after controlling for variables that have been previously shown to be related to analyst forecast errors, we find evidence that analysts’ forecast accuracy around current restructurings is significantly associated with their innate ability and task-specific restructuring experience. The magnitude of the performance benefit appears to be economically significant, as the accuracy of analysts’ forecasts around restructurings improves by over eight percent for each year of restructuring experience. Not surprisingly, we also find that when we do not include task-specific restructuring experience in our model, innate ability subsumes the measures of general and firm-specific experience—a result that is consistent with the findings of Jacob, et al. (1999).

Second, we also show an important moderating effect of innate ability on the potential for analysts to learn from their prior task-specific experiences. Specifically, we find a stronger association between forecasting performance and task-specific experience for those analysts who are considered survivors in their jobs (i.e., those who presumably have the greatest innate ability) than for non-survivors. These results suggest that analysts survive perhaps because their high innate ability allows them to better learn from their task-specific experiences.

Our paper has both theoretical and practical implications. From a theoretical perspective, we add to the existing accounting literature regarding the capability of analysts to learn from their prior experiences. Our results confirm the finding that the feedback that analysts receive from general and firm-specific experiences is not particularly useful to the task of forecasting earnings. Importantly, though, we extend the literature by showing that learning from experience can occur, but that such feedback should be at the level of the task to be beneficial to the analyst. We also demonstrate that those analysts who survive actually benefit the most from their task-specific experiences.

From a practical standpoint, our results have implications for brokerage firms and investors. Specifically, our results suggest that brokerage firms may want to consider hiring those professionals with the highest innate ability, as we show that those analysts have the best forecasting performance. Most interestingly, though, our results suggest that analysts can learn from task-specific experience, also signifying the importance of training and repetition in the kinds of tasks to which an analyst is exposed.
For investors, we provide insights to help them evaluate analyst earnings forecasts which are one factor in their decisions about allocating funds. Our new findings about task-specific restructuring experience should help them identify those analysts who are likely to provide the most-accurate forecasts, particularly during times of high uncertainty (e.g., during restructurings) when forecasts are likely to be the most useful to investors.

The remainder of the paper is organized as follows. Section 2 provides our hypothesis development. Section 3 discusses the research design, while Section 4 addresses sample selection procedures and descriptive statistics. Section 5 presents the empirical findings, and Section 6 provides conclusions.

2. Hypothesis Development

2.1 Experience and forecast accuracy

The job of analysts requires that they gather and analyze information to make a prediction of future earnings. Research has documented variation in the quality of analysts’ forecasts, with some analysts issuing more accurate forecasts than others (Sinha, Brown and Das, 1997; Stickel, 1992; Park and Stice, 2000). Some studies have found that analyst experience is important in explaining this variation in accuracy (i.e., more experience leads to greater accuracy), while other studies have found no such relation. Specifically, Clement (1999) documents that general experience, defined as the tenure of the analyst (i.e., years of analyst work experience) is important in explaining forecast accuracy. Mikhail, et al. (1997) show that another type of experience—namely, firm-specific experience (i.e., the number of prior quarters an analyst issues a quarterly earnings forecast for a specific firm)—also is relevant in explaining analyst forecast accuracy. In contrast, Jacob, et al. report that firm-specific measures of experience are not significant once they control for the analyst’s innate ability, suggesting the dominant role of innate ability in explaining analyst forecasting performance. Innate ability captures various aptitudes, such as the ability to retrieve information when needed, make inferences, analyze information, and be creative (Libby and Luft, 1993; Sternberg, 1997).

In this paper, we address these seemingly conflicting findings by documenting the type of prior experience that influences analyst forecasting accuracy. Drawing on psychology research, we posit that
the measures of experience used in prior analyst research – whether number of years of forecasting
experience as an analyst or number of years of forecasting experience with a particular firm – may be too
general, as the analyst may not be able to take the knowledge gleaned from those experiences and transfer
it to a new, novel situation not previously experienced (or not experienced on a repeated basis). Indeed,
research demonstrates that unless individuals have specific experiences at a task that is very similar to the
new task at hand, prior experiences may not be relevant nor helpful.4

This hypothesis is conceptually appealing as measures of general, firm-specific, and task-specific
experience arguably capture different types of experiences and, thus, lead to different types of knowledge
(Chi, et al., 1988; Bonner and Lewis, 1990; Davis and Solomon, 1989). That is, analysts with the same
number of years of general experience are likely to have very different specific experiences – both with
the firms that they follow and with the situations and events of those firms. Likewise, analysts with the
same number of years of firm experience are also likely to have different specific experiences. For
example, one analyst may have ten years of experience following Firms A, B and C, while the other
analyst may have ten years of experience following Firms X, Y and Z. Although both analysts have the
same level of general and firm-specific experience, each may have exposure to different events for those
firms. Firms A, B and C may have undertaken a number of leveraged-buyouts during the ten-year
coverage period, while Firms X, Y and Z may have undertaken none. Given this, the analyst covering
Firms A, B and C would have higher task-specific experience in leveraged buyouts than the analyst
covering Firms X, Y and Z.

In sum, based on the prior psychology research, we hypothesize that task-specific experience, but not
general or firm-specific experience, will positively enhance the forecasting performance of analysts when
controlling for the innate ability of the analyst.

2.2 The joint effects of task-specific experience and innate ability

Most models of learning indicate the centrality of both innate ability and experience in understanding
performance at a variety of tasks. For example, Bonner and Lewis (1990) show that innate ability and
experience are likely to have main effects on performance. That is, greater innate ability and greater
experience are likely to enhance performance, with each exerting independent effects on performance. This perspective is consistent with the ideas already discussed in this paper.

We take this idea one step further, though, by drawing on psychology theory that maintains that there should be an interactive relationship between ability and experience. That is, this research indicates that ability may facilitate the acquisition of knowledge gained from experience which, in turn, will have a positive performance impact (Ackerman, 1989; Bonner, 2006; Hunter, 1986; Libby and Luft, 1993). Said another way, those with higher (lower) ability may be most (least) likely to learn from their experiences. The basic premise behind this idea is that high-ability individuals have greater cognitive capacity which, in turn, allows them to achieve greater performance gains from their task-specific experiences. Thus, we also hypothesize that task-specific experience may improve analysts’ forecasting performance around restructurings to a greater extent for those analysts with high ability versus those with lower ability.

2.4 Where to test for task-specific experience effects?

The task we focus on pertains to restructuring charges associated with firm downsizings. Restructuring charges from downsizings are costs to exit activities. They include, for example, costs to close plants, sell off assets, terminate employees, cancel leases, and so on. Although there are other events that are given the restructuring label (e.g., divisional reconfigurations, unit mergers, management terminations), we focus only on those associated with downsizings. We intentionally made this design choice to ensure that the task was one that analysts could experience repeatedly, was relatively homogenous, and was difficult enough to allow those with greater task-specific experience to demonstrate improved forecasting performance.

When a firm has a restructuring associated with downsizing, the cost of implementing the restructuring plan is recorded as a charge against income in the year of the restructuring decision. These plans are most often implemented over at least one subsequent period, thereby affecting the earnings of those subsequent periods. Although there is little consensus in the extant literature on the long-term performance implications of downsizing restructurings, this same literature reports very consistent results on the near-term (year+1) earnings effect of restructurings (which is our focus in this study). Specifi-
cally, prior research documents that downsizing restructurings have a significantly negative effect on earnings in the year following the announcement of the event (Holder-Webb, et al., 2005; Atiase, et al., 2004; Carter, 2000; Brickley and Van Drunen, 1990).5

The impact of a charge on subsequent earnings makes it difficult for the analyst to forecast those subsequent earnings for at least two reasons (Alford and Berger, 1999; Chaney, et al., 1999). First, the magnitude of any economic ramifications of a restructuring are not always apparent at the time the decision to restructure is made. For example, a restructuring involving a workforce reduction may signal future efficiency gains from lower future payroll expenses, but it also could signal a reduction in future productivity and/or demand. Second, the future implications of restructuring charges can be complicated by the discretion in the restructuring charge itself as well as the discretion available for other accounting choices. For example, some firms intentionally minimize restructuring charges in a given year to keep earnings from being penalized, while others will maximize the amount of the charge to communicate the bad news all at once (i.e., take a big bath). The forecasting difficulties associated with this discretion are compounded when considering the other accounting choices made by companies. For example, firms that make conservative accounting choices, such as those associated with accelerated depreciation, will have smaller amounts to write off as restructuring charges for an impaired asset (and, thus, higher future earnings) as compared to those who use less-conservative methods, such as straight-line depreciation.

Although a prior study by Chaney, et al. (1999) provides some evidence regarding whether analysts learn from prior restructuring charges, learning-from-experience was not the primary purpose of their study. These authors find that the existence of a prior restructuring within the past two years of a current restructuring is not associated with improved consensus forecast accuracy. Although this result appears to contradict our hypothesis (and findings), Chaney, et al. examined consensus forecast accuracy (i.e., firm-level accuracy across all analysts who follow that firm) rather than individual analyst accuracy, as we do. Their focus on consensus accuracy precludes them from controlling for a number of analyst-specific and forecast-specific factors, such as forecast horizon, number of companies followed, and so on, that have been previously associated with forecast accuracy (Clement, 1999; Mikhail, et al. 1997; Jacob,
et al. 1999). Thus, one possible reason for their not finding a relationship between consensus accuracy and prior restructuring experience is their inability to incorporate such control variables.6

3. Research Design

3.1 Overview

Our primary hypothesis is that analyst earnings forecast accuracy improves with both innate ability and task-specific experience. We define earnings as net income before special items and one-time events (e.g., restructuring charges). The conceptual model from which our empirical tests are derived is:

\[ \text{Analyst Forecast Accuracy} = f(\text{innate ability, task-specific experience, control variables}) \]  

(1)

The control variables are documented in prior research as being associated with analyst forecast accuracy. Specifically, forecast performance improves with the size of the analysts’ employer (Clement, 1999), the frequency of forecast revisions (Jacob, et al., 1999), and prior forecast accuracy (Park and Stice, 2000; Brown, 2001). Forecast accuracy deteriorates with forecast horizon and the number of firms and industries followed (Clement, 1999). In addition to using these variables, we also include industry specialization to control for the possibility that there is industry clustering in restructuring charges.

Our empirical analysis has two parts. First, we investigate whether task-specific experience positively influences forecast performance around periods that involve a current restructuring and does not affect forecast performance (as much) around periods not involving a restructuring, even when the innate ability of the analyst is considered. That is, we test for a potential interaction between task-specific experience and whether or not the current period involves a restructuring. Second, once the importance of task-specific experience is established, we then explore the notion that those analysts who survive the longest in the industry and, thus, presumably have the highest innate ability are more likely to learn from their task-specific experiences than those who do not survive. That is, we test for a potential interaction between task-specific experience, whether or not the current period involves a restructuring, and whether or not the analyst is a survivor.
3.2 Effect of general experience, firm-specific experience, and innate ability on forecast accuracy

We begin our empirical analysis with a model that includes general, firm-specific, and task-specific experience and the control variables identified earlier. This model is shown below, in equation (2):

\[
PMAFE_{ijt+1} = \alpha_1 DGEXP_{ijt} + \alpha_2 DFEXP_{ijt} + \alpha_3 DAGE_{ijt} + \alpha_4 DTOP10_{ijt} + \alpha_5 DNCOS_{ijt} + \alpha_6 DNSIC2_{ijt} + \alpha_7 DFREQ_{ijt} + \alpha_8 LPMAFE_{ijt} + \alpha_9 DIEXP_{ijt} + \alpha_{10} RC_YR_{jt} + \alpha_{11} DREXP_{ijt} + \epsilon_{ijt}
\]

where:

- \( PMAFE_{ijt+1} = (AFE_{ijt+1} - MAFE_{jt+1}) / MAFE_{jt+1} \)
- \( AFE_{ijt+1} = \) absolute forecast error for analyst \( i \)'s forecast of firm \( j \)'s earnings in year \( t+1 \).
- \( MAFE_{jt+1} = \) mean absolute forecast error for analysts following firm \( j \) in year \( t+1 \).
- \( DGEXP_{ijt} = \) the (mean-adjusted) general forecasting experience is number of years (including year \( t \)) that analyst \( i \) supplied a forecast.
- \( DFEXP_{ijt} = \) the (mean-adjusted) firm-specific experience is number of years (including year \( t \)) that analyst \( i \) supplied a forecast for firm \( j \).
- \( DAGE_{ijt} = \) the (mean-adjusted) age of analyst \( i \)'s forecast for firm \( j \) in year \( t \), where age is the number of days from the forecast date to 30 days prior to fiscal period end.
- \( DTOP10_{ijt} = \) the (mean-adjusted) dummy variable with value of 1 if analyst \( i \) works at a top decile broker (in terms of number of employees) in year \( t \) and 0 otherwise.
- \( DNCOS_{ijt} = \) the (mean-adjusted) number of firms followed by analyst \( i \) in year \( t \).
- \( DNSIC2_{ijt} = \) the (mean-adjusted) number of two-digit SICs followed by analyst \( i \) for firm \( j \) in year \( t \).
- \( DFREQ_{ijt} = \) the (mean-adjusted) number of forecast revisions supplied by analyst \( i \) for firm \( j \) in year \( t \).
- \( LPMAFE_{ijt} = \) PMAFE for year \( t \).
- \( DIEXP_{ijt} = \) the (mean-adjusted) percentage of firms followed by analyst \( i \) in year \( t \) in the same 2-digit SIC code as firm \( j \).
- \( RC_YR_{jt} = 1 \) if year \( t \) is a restructuring year, otherwise zero.
- \( DREXP_{ijt} = \) the mean-adjusted number of restructurings analyst \( i \) has encountered for firm \( j \) in the five years prior to year \( t \).
- \( DREXP_{ijt} \times RC_YR_{jt} = \) the mean-adjusted number of restructurings analyst \( i \) has encountered for firm \( j \) in the five years prior to year \( t \), if year \( t \) is a restructuring year, otherwise zero.

There are two important features of our model. First, we mean-adjust all variables (cf. Brown, 2001; Clement, 1999; Jacob, et al., 1999). That is, the model is estimated in the form of:

\[
Y_{ijt} - \bar{Y}_j = (X_{ijt} - \bar{X}_j) \beta + \epsilon_{ijt}
\]

The mean-adjustment procedure controls for firm-year fixed effects and is econometrically equivalent to including firm-year dummy variables in the model (see Greene, 1990, 485). This approach allows us to compare analyst forecast accuracy and various analyst characteristics to...
the forecast accuracy and characteristics of other analysts who supplied a forecast for the same firm during the same time period. Specifically, the mean-adjustment procedure allows us to test our ideas across firms and time periods without the possible contaminating effects of, for example, variations in forecasting difficulty that arise from differences in the level and/or type of restructuring disclosures in any given period and for any given firm. A positive value for our dependent measure, \( PMAFE \), suggests worse than average performance while a negative value suggests better than average performance.\(^7\)

Second, because our analysis focuses on the association between task-specific experience and subsequent accuracy, we use period \( t+1 \) forecasting performance as the dependent variable. In other words, we investigate how analysts’ characteristics are associated with one-year-ahead forecasting performance.

Our primary independent variable of interest in this model is the interaction term – \( DREXP \times RC_{YR} \). With this variable, we test the idea that the knowledge gained from previous restructuring experience will transfer to a new restructuring situation. Thus, the expected negative coefficient on this interaction term would be interpreted as the percentage improvement in an analyst’s forecast accuracy for each year of restructuring experience relative to other analysts who followed the same stock that year.

In terms of the \( DREXP \) and \( RC_{YR} \) variables, we expect both to be statistically insignificant. The coefficient on \( DREXP \) is expected to be insignificant given that restructuring experience is unlikely to provide beneficial information to analysts in non-restructuring periods. That is, the task-specific experiences of analysts (i.e., prior restructurings) are only expected to be useful in forecasting around subsequent occurrences of the same task (restructurings). The coefficient on \( RC_{YR} \) also is expected to be insignificant due to our mean-adjustment procedure. That is, by subtracting out the mean forecast accuracy of all analysts issuing a forecast for that particular firm in that particular period, we remove the deterioration in forecast accuracy one would expect to see with a current restructuring (Alford and Berger, 1999; Chaney et al., 1999). In terms of predictions for our primary and control variables, we rely on prior research and predict positive coefficients for the following control variables, \( DAGE, DNCOS, DNSIC2 \) and \( LPMAFE \), and negative coefficients for \( DGEXP, DFEXP, DTOP10, DFREQ \), and \( DIEXP \).
4. Sample Selection, Data Sources, and Descriptive Statistics

4.1 Sample selection and data sources

The data for this study is obtained from various sources. We obtain an initial sample of downsizing restructuring observations by searching the NAARS (National Automated Accounting Research System) database for the years 1983 to 1994 using the search strings *restruct!*, *unusual!*, and *special!*. For the years 1995 to 2003, the Lexis-Nexis Academic Universe Business News database is searched using the same search strings. With this preliminary sample, we then reviewed the annual financial statements and footnotes of each firm to identify only those that took a restructuring charge related to downsizing. As noted earlier, we limited our sample to downsizing restructurings to hold constant (to the extent possible) the relative complexity and other features of the task. Focusing on a single type of restructuring increases the homogeneity of our sample which, in turn, facilitates detecting a relationship between experience and performance, should one exist. Because of this feature of our design, examination of the footnotes is necessary to ensure a reliable sample of downsizing observations.8

We collect analyst forecast data and actual earnings for each firm in the sample from the Institutional Broker Estimate System (I/B/E/S) Detail History file. I/B/E/S adjusts actual earnings for nonrecurring items, such as restructurings, to reflect the firms’ ongoing operating earnings and for consistency with the earnings construct being forecast by analysts. We retain in our sample only the last forecast for each analyst-firm-year pairing with a minimum forecast horizon of 30 days (cf. Clement, 1999). Further, we require that the forecast be issued in the first eleven months of the firms’ fiscal year, thereby imposing a maximum forecast horizon. To implement the mean-adjustment procedure, we also require a minimum of two analysts per firm issue a forecast in a period to be included in the sample (cf. Clement, 1999). Although this procedure requires that we have at least two analysts following a firm for a particular year, we believe that its benefits outweigh the relatively minor reduction in sample size that it causes.9

Other financial data are taken from Compustat, CRSP, or I/B/E/S for each year available from 1983 though 2004. Thus, there are a maximum of 22 years data for each firm. We eliminate observations for
firms not covered on I/B/E/S, Compustat, or CRSP and for missing data items on these databases. This results in a final test sample of 2,204 restructuring observations taken by 657 firms.

4.2 Descriptive statistics

A descriptive analysis of our entire sample (not tabulated) reveals that the year with the most restructuring observations is 1993 with 248 restructurings, while the year with the fewest is 1983 with only two restructurings. We also observe that approximately 30 percent of the sample (194 firms) recognizes a single restructuring in the years 1983-2003, while over 50 percent of the sample records three or more charges during the same time period.

Table 1 reports distribution statistics and correlation coefficients for the regression variables used in equations (2). Panel A shows the distribution of unadjusted regression variables, while Panel B shows the adjusted regression variables. Turning first to the unadjusted variables, we observe that these medians values are consistent with results documented in prior research. For example, the median values for $GEXP$, $FEXP$ and $FREQ$ are five, three, and three, respectively, which is consistent with comparable data reported by Clement and Tse (2005). We note that the median age of the forecast ($DAGE = 65$ days), the median value for the number of companies followed ($NCOS = 16$) and the number of companies followed under a two-digit SIC code ($NSIC2 = 4$) also are comparable to their study. Panel B of Table 1 shows distributions of the mean-adjusted regression variables which are used in our empirical tests. Means are not reported because our mean-adjustment procedure forces the means to be zero. We observe that the median values for $PMAFE$, $DAGE$, $DNCOS$, and $DNSIC2$ are less than means for these variables. These results are consistent with comparable data reported by Clement (1999).

| Insert Table 1 here |

Finally, Panel C presents a correlation matrix for our mean-adjusted variables. Consistent with prior research, forecast errors ($PMAFE$) are negatively correlated with general and firm-specific forecasting experience, employer size, forecasting frequency, and positively correlated with the number of firms and industries followed by the analyst, forecast age, and prior forecasting accuracy (Clement, 1999; Mikhail,
et al. 1997; Jacob, et al. 1999; Park and Stice, 2000; and Brown, 2001). As expected, forecast errors are negatively correlated with prior restructuring (i.e., task-specific) experience in periods of restructuring and with industry specialization. While each of these correlations has the expected sign, the multivariate tests in the following section are more appropriate for investigating the relationship between these variables.

5. Empirical Results

5.1 Results regarding task-specific experience on forecast accuracy

Before investigating the relationship between task-specific experience and forecasting performance, we replicate the prior research findings by Clement (1999), Mikhail, et al. (1997) and Jacob, et al. (1999) regard innate ability, general experience, and firm-specific experience using our restructuring sample. Doing so ensures that any new insights about task-specific experience from our primary tests cannot be attributed to the different sample used herein.10

Turning first to the replication tests, Column 1 of Table 2 presents the results from the estimation of equation (2) excluding the DREXP, RC_YR, and DREXP×RC_YR variables and the dummy variables which capture innate ability. Column 2 shows comparable test results including the innate ability dummy variables. These results reveal similar conclusions as found in prior research with our unique sample of firms experiencing restructuring charges. Specifically, when we do not consider the innate ability of analysts (Column 1), we find that they appear to learn from their general and firm-specific experiences. We find that the coefficients on DFEXP (firm-specific analyst experience) and DGEXP (general analyst experience) are negative and significant (both p-values < 0.02).11 These results are consistent with Mikhail et al. (1997) and Clement (1999). When we do include the dummy variables designed to represent analysts’ innate ability, though, we see the DFEXP and DGEXP variables become insignificant. These Column 2 results are consistent with Jacob, et al. (1999) who report that once innate ability is considered, firm-specific experiences are not relevant to understanding analyst forecasting performance.12

In sum, these tests replicate prior research findings and suggest that any new insights obtained from our primary tests (described below) are not due to our sample composition.
Turning to our primary tests, we see that the signs and significance levels of the variables in Columns 3 and 4 are similar to those in Columns 1 and 2, respectively. Most relevant to our purposes, though, are the coefficients on the variables added to these tests – namely, the $DREXP$, $RC_YR$, and $DREXP \times RC_YR$ variables. Turning first to the Column 3 results that do not include the dummy variables designed to capture innate ability, we see that the coefficients on $DREXP$ and $RC_YR$ are not significantly different than zero, as expected. That is, the mean-adjustment procedure removes the deterioration one would expect to see with a current restructuring, thereby logically causing the $RC_YR$ variable to be insignificant. Also, the insignificant coefficient on $DREXP$ (main effect) suggests that restructuring experience is not helpful in non-restructuring years, as we hypothesized. Of primary interest is the interaction coefficient which is negative and statistically significant (-0.1101; $p$-value < 0.01). These results indicate that restructuring experience improves forecast accuracy more in a restructuring year than in a non-restructuring year, consistent with our predictions. Stated another way, the knowledge gained from previous restructuring experience will transfer to a new restructuring situation that is encountered but will not transfer to other non-restructuring related situations. In sum, this evidence suggests that analysts are able to learn from experience with past restructuring events.

To ensure that our measure of task-specific experience is not merely capturing the effects of innate ability, we re-estimate equation (2) now including variables capturing analysts’ innate ability. Under our hypothesis, we expect the interaction variable to remain significant even after including the innate ability variables. The results of this additional test are reported in Column 4. As expected, the coefficient on the $DREXP \times RC_YR$ variable remains negative and significant (-0.0845; $p$-value < 0.01), and the coefficients on the $DREXP$ and $RC_YR$ variables remain statistically insignificant. That is, even after including the variables designed to capture the analyst’s innate ability, we still see analyst performance being positively associated with their task-specific experience during restructuring periods and not associated with it during non-restructuring periods. When restructurings do occur, the relative forecast accuracy for the
subsequent year improves at roughly 8.5 percent for each year of firm-specific restructuring experience.\textsuperscript{15} Taken together, these results are consistent with our prediction.

5.2 Robustness tests

We subject this conclusion to several robustness tests. First, to control for the cross-sectional correlation in analyst forecast errors, we estimate 21 annual Fama and MacBeth (1973) regressions of Equation (2) (i.e., one for each year between 1983 and 2003). The results of these additional tests, not tabulated, are consistent with those reported in Table 2. We find that the coefficient on the interaction term, $DREXP \times RC\_YR$, is negative in 17 out of 21 years estimated, and the average interaction coefficient is negative (-0.064) and significant ($p$-value < 0.01).

Second, to control for potential autocorrelation of analyst forecast errors, we used the Newey-West (1987) method to estimate robust standard errors for the coefficient estimates from the Fama-MacBeth regressions. In addition, we controlled for serial correlation of each individual analyst’s forecasts for a particular firm in our pooled regressions using a generalized estimating equations procedure discussed in Chaganty (1997). The results of both of these additional analyses (not tabulated) are consistent with our tabulated results.

Third, because our mean-adjustment procedure for $DREXP$ often yields values of zero, it is possible that our results are driven by a few sample firms or analysts. Consequently, we re-estimated Equation (2) after dropping firms with fewer than two restructurings (194 firms). Results reveal that our conclusions remain unchanged, indicating that the zero values for $DREXP$ do not systematically influence our results.\textsuperscript{16}

Fourth, we test the robustness of our results by examining those situations where analysts have fewer opportunities to gain task-specific experience. Specifically, we re-estimate Equation (2) on the subset of firms that report only two or three restructurings during the sample period (231 firms). The results of this test, not tabulated, are similar to those reported in Table 2. We also test this idea by using alternative measures of $DREXP$. We change the specification of $DREXP$ to measure prior restructuring experience in the prior two, three, and four years (versus five years as in our primary tests) and then re-estimate
equation (2). The results of these tests (not tabulated) are similar to those reported in Table 2. Thus, it appears analysts are able to learn with even limited task-specific experience.

Fifth, we broaden DREXP by measuring it as general restructuring (versus firm-specific) experience. General restructuring experience is measured as the mean-adjusted number of restructurings analyst \( i \) has encountered for all firms in the five years prior to year \( t \). This variable is defined within our sample as it uses restructuring observations from only the 657 firms identified in our search from 1983-2003. The results of this additional test (not tabulated) are consistent with those reported in Table 2. Specifically, the coefficient on the interaction term using this new measure remains negative (-0.045) and significant \((p\text{-value} < 0.01)\). When restructurings occur, relative forecast accuracy improves at roughly 4.5 percent for each year of general restructuring experience. Thus, it appears that general restructuring experience, while associated with improved forecast accuracy, is not as valuable to the analyst as firm-specific restructuring experience.\(^{17}\)

We also estimate a model that includes both this general-restructuring experience measure and our original firm-specific measure of task-specific experience. The results of this test (not tabulated) indicate that the coefficient on the interaction term involving firm-specific restructuring experience remains negative (-0.0985) and significant \((p\text{-value} < 0.01)\), but the coefficient on an analogous interaction term involving general restructuring experience, though negative, is only marginally significant \((p\text{-value} = 0.10)\). This evidence suggests that the effect of general restructuring experience is subsumed by firm-specific restructuring experience. That is, analysts learn better with experience that is specific to the firm and task.

5.3 Testing the effect of innate ability on learning from task-specific experience

In this paper, we also test the hypothesis that those analysts who survive the longest and, thus, presumably have the highest innate ability are actually more likely to learn from their task-specific experiences than are those who do not survive. For this analysis, we initially define survivors as analysts who have greater than five years of firm-specific experience. Using this partition, both survivors and non-survivors have experienced a significantly positive number of prior restructurings, on average. That
is, the mean for survivors is 1.1 while the mean for non-survivors is 0.79, with both being statistically significantly greater than zero (both $p$-values $< 0.01$).

Table 3 presents the results of separately estimating equation (2) for the survivors (Column 1) and the non-survivors (Column 2). In general, the results for all variables, excluding the interaction variable, are similar to those previously reported in Table 2. Directly relevant to our purposes, though, is the sign and significance on the $DREXP \times RC_YR$ interaction variable. Consistent with our hypothesis, we find that it is negative and significant for survivors (-0.1157, $p < 0.01$) but it is not significant for non-survivors (-0.0206; $p$-value = 0.15). That is, those analysts who survive and presumably have the greatest innate ability do improve their forecasting performance around current restructurings more when they have greater amounts of task-specific restructuring experience. Further, this experience has no significant effect in non-restructuring periods, as one would expect (i.e., insignificant $RC_YR$ coefficient). In contrast, those analysts who do not survive (and presumably have less innate ability) do not appear to learn as much, if at all, from their task-specific experiences as do survivors.

To test whether the difference in the slope coefficients for the survivors and non-survivors is statistically significant, we estimate a model that builds on the one shown in equation (2). Specifically, we add another term to capture the potential three-way interaction between $DREXP$, $RC_YR$, and our newly created survivorship variable. Those analysts with greater than five years of firm-specific experience are coded as 1 and those with five or fewer years of firm-specific experience are coded as 0. Results show that the coefficient on this three-way interaction (not tabulated) is negative (-0.0836) and statistically significant ($p$-value $< 0.01$), as expected.

Robustness tests reveal that the significance of the three-way interaction remains even with other survivorship cutoffs. Specifically, the interaction remains statistically significant ($p$-value $< 0.01$) when we define survivors as having greater than three or four years of firm-specific experience. Additional robustness tests show that our results hold even when we restrict our tests to the subset of firms that report
only two or three restructurings during the sample period (231 firms).\textsuperscript{18} The results of this test, not tabulated, are similar to those reported in Table 3 column 1. We also conduct our tests dropping those observations (194 firms) with fewer than two restructurings. After estimating our models on this revised sample, we see that our conclusions remain unchanged, indicating that zero values for $DREXP$ do not significantly influence our results. Overall, these additional tests again confirm our hypothesis that those analysts with the highest innate ability are more likely than analysts with lower innate ability to leverage that ability and translate it into greater forecast accuracy when being repeatedly exposed to restructurings.

6. Conclusion

This study makes several important contributions to the literature. Specifically, our study adds to the existing literature regarding the ability of analysts to learn from their prior experiences. Our results confirm those of Jacob, et al. who find that the feedback analysts receive from general and firm-specific experiences are not particularly useful to their performance. We add to this literature by demonstrating that feedback at the task level appears to be an important contributor to our understanding of analyst performance. That is, we show that there needs to be a “match” between an analyst’s prior experiences and the current task at hand for those experiences to influence performance. Furthermore, we provide new insights to the literature by also showing that those analysts who survive the longest at the job (presumably those with the highest innate ability) are more likely to learn from their task-specific experiences than those who did not survive. These insights, obtained within the context of analysts forecasting around restructurings, are particularly important because of the important role that analysts play in investors’ investment decisions.

As with any study, there are limitations. For example, because it is impossible to know when the implications of a restructuring “end,” we are unable to determine the amount of feedback that the analyst receives from forecasting around a prior restructuring event. Although a finer measure of feedback might strengthen our results, the ordinal measure that we used is justified on the grounds that it is not clear from existing theory whether an analyst would learn different amounts about forecasting next-year earnings
from observing a restructuring charge (in the current period) that had implications for one versus several subsequent years.

Another possible limitation of our study is our focus on downsizing restructurings as the task. Even though our approach reduced the heterogeneity in our sample (and thereby provided us a powerful opportunity to detect a relationship between task-specific experience and analyst learning), it is possible that there is still some meaningful variation in the components of those downsizing restructurings. That is, analysts may learn more quickly or better from experience with, say, restructurings involving termination benefits than they do from plant closings. Indeed, we choose downsizing restructurings as our task because we believed it was neither an extremely simple nor extremely difficult task for the analyst. Thus, it is possible that our insights do not generalize to settings where it is easier or much more difficult to forecast earnings. Indeed, it is important that future research explore our insights within the context of other types of tasks, including those that vary in their difficulty. Doing so will test the generalizability of our findings and further expand our understanding of experience, ability, and analyst forecast performance.
ENDNOTES

1 In this study we refer to the Jacob et al. (1999) aptitude variable (analyst-firm alignment) as innate ability.

2 In other words, we define the task more narrowly than Mikhail, et al. (1997) who define the task as forecasting earnings for a particular firm, collapsing across all types of events or situations that occur for that firm. We refer to the latter as firm-specific experience, and reserve the term task-specific experience for forecasts made for a particular firm experiencing a particular type of event or situation.

3 Although the studies by Mikhail, et al. (1997) and Clement (1999) appear to be more similar to each other than to Jacob, et al. (1999), there are nevertheless a number of commonalities between Clement and Jacob, et al. as well as unique features of Mikhail, et al. Specifically, Clement and Jacob, et al. both use a pooled cross-sectional design in which each analyst-firm combination is required be in the sample only once (one year). On the other hand, Mikhail, et al. rely on a time-series design in which they require each analyst-firm combination be in the sample a minimum of 32 quarters (8 years). Thus, Mikhail, et al. construct a sample of surviving analysts who have made forecasts across all types of situations or tasks. As discussed later in our paper, we also examine the forecasting performance of analysts who are considered survivors, but we do so within the context of forecasting around restructurings (versus across all types of tasks or situations). While Mikhail, et al. report a significant firm-specific experience effect with their sample of surviving analysts, we extend their finding by also demonstrating a task-specific experience effect for survivor analysts.

4 Although there are situations where transferring knowledge learned in one context to other contexts does occur and such transfer has a positive impact on performance (Holland, Jolyoak, Nisbett and Thagard, 1986), this research has largely concluded that the conditions for this kind of transfer are fairly restrictive (Novick, 1988; Singley and Anderson, 1989; Gick and Holyoak, 1983; Marchant, 1989).

5 We replicate this finding using our sample of restructurings. Specifically, we observe that the median return on equity and operating margin relative to the industry are significantly negative in the year after the restructuring. In addition, the size-adjusted and performance-adjusted operating margins are significantly negative for the subsequent year, as is the size-adjusted return on equity.

6 Another possible interpretation of their result is that not all of the analysts issuing a forecast for the current period followed the firm in the previous two years. That is, each of the analysts whose forecasts comprise the current consensus forecast may not have experienced the prior restructuring(s). To address this possibility, we re-estimated Chaney, et al.’s model using only those analysts who followed the firm the previous two years. These test results show no evidence of learning from experience at the firm-level (consensus analyst forecast). We are not surprised by this result because the use of the consensus forecast does not allow us to account for differences in task-specific experience and other important individual-analyst control variables that have been previously documented as important to understanding forecast accuracy.

7 We also tested the robustness of our results using another measure of forecast accuracy. Specifically, drawing on the work of on Jacob, et al. (1999, 62), we transformed the absolutely analyst forecast errors (for all analysts for a company year) into rank measures. This technique has several desirable characteristics that are very similar to those of our measure. For example, this measure allows us to compare forecast accuracy across companies and years independent of differences in forecasting difficulty. The results of this additional analysis (not reported) are consistent with our primary test results.
We were unable generate our sample using Compustat, because it does not separately tabulate downsizing restructurings. Rather, Compustat reports ‘special items’ which includes different types of charges, only some of which are restructurings.

The requirement to have two analysts following a firm causes less than two-percent reduction in our sample size (1069 observations).

Additional analysis at the firm level, not tabulated, reveals that restructuring charges are associated with a reduction in forecast accuracy, consistent with Alford and Berger (1999) and Chaney, et al. (1999).

One potential concern is that our control variables are not constant across our two sub-samples (i.e., analysts with and without prior restructuring experience) and, thus, they differ in some fundamental way. Our mean-adjustment procedure (described earlier) mitigates this potential problem. As an additional test, we interacted our control variables with a new dummy variable that has a value of 1 (0) when our $DREXP \times RC_YR$ measure is non-zero (zero). Each of the interactions is statistically insignificant with the exception of one interaction (with $DAGE$ – age of forecast), suggesting that our control variables are stable.

We also conducted a nested $F$-test (not tabulated) designed to ensure that the innate ability variables in the column 2 regression are jointly significant and add explanatory power to the column 1 regression. This test reveals that the innate ability variables are jointly significant ($F$-statistic = 1.30, $p < 0.01$). The $F$-statistic has 5,522 (62,527) numerator (denominator) degrees of freedom.

In addition to the reported $t$-statistics, we re-estimated our test statistics using White’s $t$-test. The results of these additional tests, not reported, are consistent with those reported in the tables.

Our primary analysis assumes that the effect of task-specific experience on forecast accuracy is linear. However, some prior work in psychology suggests that for some tasks, the relationship may be non-linear (Ericsson, 2006). To address this possibility, we log transformed an ordinal measure of prior analyst restructuring experience. We mean-adjusted this log-transformed measure and used this measure in place of our primary measure of prior restructuring experience, $DREXP$. We then re-estimated equation (2) and observed that the new log-transformed measure was negative (-0.0026) and significant at the $p < 0.01$, consistent with our primary analysis.

We also conducted a nested $F$-test (not tabulated) designed to ensure that the innate ability variables in the column 4 regression are jointly significant and add explanatory power to the column 3 regression. This test reveals that the innate ability variables are jointly significant ($F$-statistic = 1.31, $p < 0.01$). The $F$-statistic has 5,522 (62,527) numerator (denominator) degrees of freedom.

Approximately 13 percent of our sample observations (i.e., around 9,100 of the total observations) have non-zero values of $DREXP \times RC_YR$. Thus, it does not appear that our results are driven by a few sample firms or sample analysts.

An alternative potential explanation is that because our general measure of restructuring experience is defined within our sample, it is a less-powerful measure than our firm-specific task experience variable. Because of the difficulty in collecting restructuring experience for (1) all firms covered by each analyst in our sample, and (2) for time periods before 1983 (i.e., our earliest sample period), we are not able to more precisely measure this alternative measure. This inability represents a potential limitation of this test.
We limit these additional tests to surviving analysts since our primary tests in this section (see Table 3 Column 2) suggest that non-surviving analysts do not learn from their prior experiences with past restructurings.
REFERENCES


Table 1
Correlation coefficients and distributions of regression variables (n=68,058)

Panel A: Distribution of unadjusted regression variables

<table>
<thead>
<tr>
<th></th>
<th>AFE</th>
<th>GEXP</th>
<th>FEXP</th>
<th>RC_YR</th>
<th>REXP</th>
<th>REXP x RC_YR</th>
<th>AGE</th>
<th>TOP10</th>
<th>NCOS</th>
<th>NSIC2</th>
<th>FREQ</th>
<th>LAFE</th>
<th>IEXP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q1</td>
<td>0.04</td>
<td>3</td>
<td>2</td>
<td>0</td>
<td>0</td>
<td>23</td>
<td>0</td>
<td>11</td>
<td>3</td>
<td>2</td>
<td>0.01</td>
<td>0.16</td>
<td></td>
</tr>
<tr>
<td>Median</td>
<td>0.05</td>
<td>5</td>
<td>3</td>
<td>0</td>
<td>1</td>
<td>65</td>
<td>0</td>
<td>16</td>
<td>4</td>
<td>3</td>
<td>0.04</td>
<td>0.42</td>
<td></td>
</tr>
<tr>
<td>Q3</td>
<td>0.17</td>
<td>9</td>
<td>6</td>
<td>1</td>
<td>2</td>
<td>148</td>
<td>0</td>
<td>22</td>
<td>7</td>
<td>5</td>
<td>0.16</td>
<td>0.77</td>
<td></td>
</tr>
</tbody>
</table>

VARIABLE DEFINITIONS FOR PANEL A:

\[ AFE_{ijt+1} \] = absolute forecast error for analyst \( i \)'s forecast of firm \( j \)'s earnings in year \( t+1 \).

\[ GEXP_{ijt} \] = general forecasting experience is the number of years (including year \( t \)) that analyst \( i \) supplied a forecast.

\[ FEXP_{ijt} \] = firm-specific experience is the number of years (including year \( t \)) that analyst \( i \) supplied a forecast for firm \( j \).

\[ RC\_YR_{jt} \] = 1 if year \( t \) is a restructuring year, otherwise zero.

\[ REXP_{ijt} \] = the number of restructurings analyst \( i \) has encountered for firm \( j \) in the five years prior to year \( t \).

\[ REXP_{ijt} \times RC\_YR_{jt} \] = the number of restructurings analyst \( i \) has encountered for firm \( j \) in the five years prior to year \( t \), if year \( t \) is a restructuring year, otherwise zero.

\[ AGE_{ijt} \] = the age of analyst \( i \)'s forecast for firm \( j \) in year \( t \), where age is the number of days from the forecast date to 30 days prior to fiscal period end.

\[ TOP10_{ijt} \] = dummy variable with value of 1 if analyst \( i \) works at a top decile broker (in terms of number of employees) in year \( t \) and 0 otherwise.

\[ NCOS_{ijt} \] = the number of firms followed by analyst \( i \) in year \( t \).

\[ NSIC2_{ijt} \] = the number of two-digit SICs followed by analyst \( i \) in year \( t \).

\[ FREQ_{ijt} \] = frequency of forecast revision is the number of forecasts that analyst \( i \) supplied for firm for \( j \) in year \( t \).

\[ LAFE_{ijt} \] = \( AFE \) for year \( t \).

\[ IEXP_{ijt} \] = the percentage of firms followed by analyst \( i \) in year \( t \) in the same 2-digit SIC code as firm \( j \).
Table 1 (cont’d)

Panel B: Distributions of regression variables

<table>
<thead>
<tr>
<th></th>
<th>PMAFE</th>
<th>DGEXP</th>
<th>DFEXP</th>
<th>RC_YR</th>
<th>DREXP</th>
<th>DREXP × RC_YR</th>
<th>DAGE</th>
<th>DTOP10</th>
<th>DNCOS</th>
<th>DNSIC2</th>
<th>DFREQ</th>
<th>LPMAFE</th>
<th>DIEXP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q1</td>
<td>-0.65</td>
<td>-1.25</td>
<td>-0.74</td>
<td>0</td>
<td>0.00</td>
<td>0.00</td>
<td>-55.86</td>
<td>-0.17</td>
<td>-6.45</td>
<td>-1.92</td>
<td>-0.90</td>
<td>-0.71</td>
<td>-0.11</td>
</tr>
<tr>
<td>Median</td>
<td>-0.20</td>
<td>0.43</td>
<td>0.33</td>
<td>0</td>
<td>0.00</td>
<td>0.00</td>
<td>-16.50</td>
<td>-0.10</td>
<td>-1.85</td>
<td>-0.60</td>
<td>0.11</td>
<td>-0.31</td>
<td>0.00</td>
</tr>
<tr>
<td>Q3</td>
<td>0.25</td>
<td>2.21</td>
<td>1.63</td>
<td>1</td>
<td>0.00</td>
<td>0.00</td>
<td>45.17</td>
<td>0.00</td>
<td>3.67</td>
<td>1.00</td>
<td>1.29</td>
<td>0.05</td>
<td>0.13</td>
</tr>
</tbody>
</table>

VARIABLE DEFINITIONS FOR PANELS B AND C:

\[
PMAFE_{ijt+1} = \frac{(AFE_{ijt+1} - MAFE_{jt+1})}{MAFE_{jt+1}}
\]

\[
AFE_{ijt+1} = \text{absolute forecast error for analyst } i \text{'s forecast of firm } j \text{'s earnings in year } t+1
\]

\[
MAFE_{jt+1} = \text{mean absolute forecast error for analysts following firm } j \text{ in year } t+1.
\]

\[
DGEXP_{ijt} = \text{general forecasting experience is the (mean-adjusted) number of years (including year } t \text{) that analyst } i \text{ supplied a forecast.}
\]

\[
DFEXP_{ijt} = \text{firm-specific experience is the (mean-adjusted) number of years (including year } t \text{) that analyst } i \text{ supplied a forecast for firm } j.
\]

\[
RC_YR_{jt} = 1 \text{ if year } t \text{ is a restructuring year, otherwise zero.}
\]

\[
DREXP_{ijt} = \text{the (mean-adjusted) number of restructurings analyst } i \text{ has encountered for firm } j \text{ in the five years prior to year } t.
\]

\[
DREXP_{ijt} \times RC_YR_{jt} = \text{the (mean-adjusted) number of restructurings analyst } i \text{ has encountered for firm } j \text{ in the five years prior to year } t, \text{ if year } t \text{ is a restructuring year, otherwise zero.}
\]

\[
DAGE_{ijt} = \text{the (mean-adjusted) age of analyst } i \text{'s forecast for firm } j \text{ in year } t, \text{ where age is the number of days from the forecast date to 30 days prior to fiscal period end.}
\]

\[
DTOP10_{ijt} = \text{the (mean-adjusted) dummy variable with value of 1 if analyst } i \text{ works at a top decile broker (in terms of number of employees) in year } t \text{ and 0 otherwise.}
\]

\[
DNCOS_{ijt} = \text{the (mean-adjusted) number of firms followed by analyst } i \text{ in year } t.
\]

\[
DNSIC2_{ijt} = \text{the (mean-adjusted) number of two-digit SICs followed by analyst } i \text{ in year } t.
\]

\[
DFREQ_{ijt} = \text{frequency of forecast revision is the (mean-adjusted) number of forecasts that analyst } i \text{ supplied for firm for } j \text{ in year } t.
\]

\[
LPMAFE_{ijt} = PMAFE \text{ for year } t.
\]

\[
DIEXP_{ijt} = \text{the (mean-adjusted) percentage of firms followed by analyst } i \text{ in year } t \text{ in the same 2-digit SIC code as firm } j.
\]
Table 1 (cont’d)

Panel C: Pearson correlation coefficients

<table>
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<tr>
<th></th>
<th>PMAFE</th>
<th>DGEXP</th>
<th>DFEXP</th>
<th>RC_YR</th>
<th>DREXP</th>
<th>DREXP × RC_YR</th>
<th>DAGE</th>
<th>DTOP10</th>
<th>DNCOS</th>
<th>DNSIC2</th>
<th>DFREQ</th>
<th>LPMAFE</th>
<th>DIEXP</th>
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</thead>
<tbody>
<tr>
<td>PMAFE</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DGEXP</td>
<td>-0.013*</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DFEXP</td>
<td>-0.031*</td>
<td>0.649*</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
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</tr>
<tr>
<td>RC_YR</td>
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<td>-0.003</td>
<td>1.00</td>
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<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DREXP</td>
<td>0.014</td>
<td>0.007*</td>
<td>0.009*</td>
<td>0.160*</td>
<td>1.00</td>
<td></td>
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<td></td>
</tr>
<tr>
<td>DREXP × RC_YR</td>
<td>-0.068*</td>
<td>0.010*</td>
<td>0.003</td>
<td>0.174*</td>
<td>0.697*</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>DAGE</td>
<td>0.410*</td>
<td>-0.016*</td>
<td>-0.018*</td>
<td>-0.004</td>
<td>0.102*</td>
<td>0.169*</td>
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<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>DTOP10</td>
<td>-0.031*</td>
<td>0.044*</td>
<td>0.040*</td>
<td>0.001</td>
<td>0.004</td>
<td>0.002</td>
<td>0.009</td>
<td>1.00</td>
<td></td>
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<tr>
<td>DNCOS</td>
<td>0.015*</td>
<td>0.184*</td>
<td>0.114*</td>
<td>0.002</td>
<td>0.002</td>
<td>-0.006*</td>
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<td>0.104*</td>
<td>0.038*</td>
<td>0.000</td>
<td>0.005</td>
<td>0.002</td>
<td>0.009*</td>
<td>-0.055*</td>
<td>0.684*</td>
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<td>0.024*</td>
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<td>LPMAFE</td>
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<td>0.010</td>
<td>0.006</td>
<td>0.006</td>
<td>0.040*</td>
<td>-0.003</td>
<td>0.035*</td>
<td>0.047*</td>
<td>-0.073*</td>
<td>1.00</td>
<td></td>
<td></td>
</tr>
<tr>
<td>DIEXP</td>
<td>-0.047*</td>
<td>-0.059*</td>
<td>0.008*</td>
<td>0.001</td>
<td>-0.012*</td>
<td>-0.007*</td>
<td>-0.040*</td>
<td>0.092*</td>
<td>-0.394*</td>
<td>-0.579*</td>
<td>0.091*</td>
<td>-0.042*</td>
<td>1.00</td>
</tr>
</tbody>
</table>

*Statistically significant at the $p < 0.05$ level, two-tailed test.
Table 2
Tests of general experience, firm-specific experience, innate ability and task-specific experience on forecast accuracy for restructuring firms (n=68,058)\textsuperscript{a}

Eq (2):  \( \text{PMAFE}_{ij,t+1} = \alpha_1 \text{DGEXP}_{ij,t} + \alpha_2 \text{DFEXP}_{ij,t} + \alpha_3 \text{DAGE}_{ij,t} + \alpha_4 \text{DTOP10}_{ij,t} + \alpha_5 \text{DNCOS}_{ij,t} + \alpha_6 \text{DNSIC2}_{ij,t} + \alpha_7 \text{DFREQ}_{ij,t} + \alpha_8 \text{LPMAFE}_{ij,t} + \alpha_9 \text{DIEXP}_{ij,t} + \alpha_{10} \text{RC}_Y R_{jt} + \alpha_{11} \text{DREXP}_{ij,t} + \alpha_{12} \text{DREXP}_{ij,t} \times \text{RC}_Y R_{jt} + \epsilon_{ij,t} \)

<table>
<thead>
<tr>
<th></th>
<th>Column 1 Equation (2) excluding the task-specific experience variables and excluding dummy variables capturing analysts’ innate ability</th>
<th>Column 2 Equation (2) excluding the task-specific experience variables but including dummy variables capturing analysts’ innate ability</th>
<th>Column 3 Equation (2) excluding the dummy variables capturing analysts’ innate ability</th>
<th>Column 4 Equation (2) including the dummy variables capturing analysts’ innate ability</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \text{DGEXP} )</td>
<td>-0.0064 [0.01]</td>
<td>0.0005 [0.63]</td>
<td>-0.0063 [0.01]</td>
<td>0.0005 [0.73]</td>
</tr>
<tr>
<td>( \text{DFEXP} )</td>
<td>-0.0052 [0.01]</td>
<td>-0.0012 [0.24]</td>
<td>-0.0057 [0.01]</td>
<td>-0.0012 [0.25]</td>
</tr>
<tr>
<td>( \text{DAGE} )</td>
<td>0.0043 [0.00]</td>
<td>0.0039 [0.00]</td>
<td>0.0043 [0.00]</td>
<td>0.0039 [0.00]</td>
</tr>
<tr>
<td>( \text{DTOP10} )</td>
<td>-0.0825 [0.00]</td>
<td>-0.0071 [0.64]</td>
<td>-0.0819 [0.00]</td>
<td>-0.0071 [0.64]</td>
</tr>
<tr>
<td>( \text{DNCOS} )</td>
<td>0.0018 [0.05]</td>
<td>0.0040 [0.00]</td>
<td>0.0019 [0.04]</td>
<td>0.0040 [0.00]</td>
</tr>
<tr>
<td>( \text{DNSIC2} )</td>
<td>0.0040 [0.04]</td>
<td>0.0018 [0.50]</td>
<td>0.0040 [0.05]</td>
<td>0.0018 [0.50]</td>
</tr>
<tr>
<td>( \text{DFREQ} )</td>
<td>-0.0621 [0.00]</td>
<td>-0.0587 [0.00]</td>
<td>-0.0626 [0.00]</td>
<td>-0.0588 [0.00]</td>
</tr>
<tr>
<td>( \text{LPMAFE} )</td>
<td>0.0504 [0.00]</td>
<td>0.0067 [0.00]</td>
<td>0.0531 [0.00]</td>
<td>0.0067 [0.00]</td>
</tr>
<tr>
<td>( \text{DIEXP} )</td>
<td>-0.0968 [0.00]</td>
<td>-0.1269 [0.00]</td>
<td>-0.0962 [0.00]</td>
<td>-0.1268 [0.00]</td>
</tr>
<tr>
<td>( \text{RC}_Y R )</td>
<td>0.0017 [0.41]</td>
<td>0.0099 [0.26]</td>
<td>0.0017 [0.41]</td>
<td>0.0099 [0.26]</td>
</tr>
<tr>
<td>( \text{DREXP} )</td>
<td>0.0007 [0.62]</td>
<td>0.0039 [0.28]</td>
<td>0.0007 [0.62]</td>
<td>0.0039 [0.28]</td>
</tr>
<tr>
<td>( \text{DREXP} \times \text{RC}_Y R )</td>
<td>-0.1101 [0.00]</td>
<td>-0.0845 [0.01]</td>
<td>-0.1101 [0.00]</td>
<td>-0.0845 [0.01]</td>
</tr>
<tr>
<td>( \text{Adj-R}^2 )</td>
<td>0.1883 []</td>
<td>0.2997 []</td>
<td>0.1885 []</td>
<td>0.3002 []</td>
</tr>
</tbody>
</table>

\textsuperscript{a}Two-tailed \( p \)-values are provided in brackets below the coefficient estimates.
Table 2 (cont’d)

NOTE: In the regressions reported in columns 2 and 4, we add analyst-firm dummy variables that take the value of 1 for all forecasts made by a given analyst for a specific firm and 0 otherwise. A total of 5,522 analyst-firm dummy variables are added. These dummy variables are intended to capture analysts’ innate-ability.

VARIABLE DEFINITIONS:

\[
PMAFE_{ijt+1} = \frac{AFE_{ijt+1} - MAFE_{jt+1}}{MAFE_{jt+1}}
\]

\[AFE_{ijt+1}\] = absolute forecast error for analyst i’s forecast of firm j’s earnings in year \(t+1\).

\[MAFE_{ijt+1}\] = mean absolute forecast error for analysts following firm j in year \(t+1\).

\[DFEXP_{ijt}\] = firm-specific experience is the (mean-adjusted) number of years (including year \(t\)) that analyst i supplied a forecast for firm j.

\[DGEXP_{ijt}\] = general forecasting experience is the (mean-adjusted) number of years (including year \(t\)) that analyst i supplied a forecast.

\[DAGE_{ijt}\] = the (mean-adjusted) age of analyst i’s forecast for firm j in year \(t\), where age is the number of days from the forecast date to 30 days prior to fiscal period end.

\[DTOP10_{ijt}\] = the (mean-adjusted) dummy variable with value of 1 if analyst i works at a top decile broker (in terms of number of employees) in year \(t\) and 0 otherwise.

\[DNCOSS_{ijt}\] = the (mean-adjusted) number of firms followed by analyst i in year \(t\).

\[DNSIC2_{ijt}\] = the (mean-adjusted) number of two-digit SICs followed by analyst i in year \(t\).

\[DFREQ_{ijt}\] = frequency of forecast revision is the (mean-adjusted) number of forecasts that analyst i supplied for firm for j in year \(t\).

\[LPMAFE_{ijt}\] = \(PMAFE\) for year \(t\).

\[DIEXP_{ijt}\] = the (mean-adjusted) percentage of the firms followed by analyst i in year \(t\) in the same 2-digit SIC code as firm j.

\[RC_{YRjt}\] = 1 if year \(t\) is a restructuring year, otherwise zero.

\[DREXP_{ijt}\] = the (mean-adjusted) number of restructurings analyst i has encountered for firm j in the five years prior to year \(t\).

\[DREXP_{ijt} \times RC_{YRjt}\] = The (mean-adjusted) number of restructurings analyst i has encountered for firm j in the five years prior to year \(t\), if year \(t\) is a restructuring year, otherwise zero.
Table 3
Tests of task-specific experience and innate ability on forecast accuracy for restructuring on forecast accuracy—Partitioned by survivor and non-survivor sub-samples of analysts

Eq (2): $PMAFE_{ijt+1} = \alpha_1 DGEXP_{ijt} + \alpha_2 DFEXP_{ijt} + \alpha_3 DAGE_{ijt} + \alpha_4 DTOP10_{ijt} + \alpha_5 DNCOS_{ijt} + \alpha_6 DNSIC2_{ijt} + \alpha_7 DFREQ_{ijt} + \alpha_8 LPMAFE_{ijt} + \alpha_9 DIEXP_{ijt} + \alpha_{10} RC_{YR} + \alpha_{11} DREXP_{ijt} + \alpha_{12} DREXP_{ijt} \times RC_{YR} + \epsilon_{ijt}$

<table>
<thead>
<tr>
<th></th>
<th>(Col. 1) Surviving Analysts</th>
<th>(Col. 2) Non-Surviving Analysts</th>
</tr>
</thead>
<tbody>
<tr>
<td>$DGEXP$</td>
<td>0.0004</td>
<td>0.0096</td>
</tr>
<tr>
<td></td>
<td>[0.66]</td>
<td>[0.21]</td>
</tr>
<tr>
<td>$DFEXP$</td>
<td>-0.0017</td>
<td>0.0006</td>
</tr>
<tr>
<td></td>
<td>[0.54]</td>
<td>[0.37]</td>
</tr>
<tr>
<td>$DAGE$</td>
<td>0.0036</td>
<td>0.0044</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.00]</td>
</tr>
<tr>
<td>$DTOP10$</td>
<td>-0.0112</td>
<td>-0.0035</td>
</tr>
<tr>
<td></td>
<td>[0.48]</td>
<td>[0.85]</td>
</tr>
<tr>
<td>$DNCOS$</td>
<td>0.0047</td>
<td>0.0032</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.01]</td>
</tr>
<tr>
<td>$DNSIC2$</td>
<td>0.0011</td>
<td>0.0001</td>
</tr>
<tr>
<td></td>
<td>[0.68]</td>
<td>[0.99]</td>
</tr>
<tr>
<td>$DFREQ$</td>
<td>-0.0505</td>
<td>-0.0700</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.00]</td>
</tr>
<tr>
<td>$LPMAFE$</td>
<td>0.0076</td>
<td>0.0050</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.01]</td>
</tr>
<tr>
<td>$DIEXP$</td>
<td>-0.1327</td>
<td>-0.1220</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.00]</td>
</tr>
<tr>
<td>$RC_{YR}$</td>
<td>-0.0017</td>
<td>0.0193</td>
</tr>
<tr>
<td></td>
<td>[0.85]</td>
<td>[0.67]</td>
</tr>
<tr>
<td>$DREXP$</td>
<td>0.0046</td>
<td>0.0028</td>
</tr>
<tr>
<td></td>
<td>[0.21]</td>
<td>[0.66]</td>
</tr>
<tr>
<td>$DREXP \times RC_{YR}$</td>
<td>-0.1157</td>
<td>-0.0206</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.15]</td>
</tr>
<tr>
<td>$ADJ-R^2$</td>
<td>0.1825</td>
<td>0.4440</td>
</tr>
</tbody>
</table>

Two-tailed $p$-values are provided in brackets below the coefficient estimates.

NOTE: In both regressions reported above, we add analyst-firm dummy variables that take the value of 1 for all forecasts made by a given analyst for a specific firm and 0 otherwise. A total of 1,466 and 4,056 analyst-firm dummy variables are added to the regressions in Columns 1 and 2, respectively. These dummy variables are intended to capture analysts’ innate-ability. All other variables are defined in Table 2.